

Commentary

# Looking Back

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## Looking Back

The opportunity to participate in this Festschrift provides an irresistible temptation to look back to the early 1950s when the Institute of Environmental Medicine was established and Norton Nelson's pivotal role in its development took shape. My participation was limited to four happy and productive years in the Institute from 1951 to 1955. It began with an invitation from Anthony Lanza and Norton Nelson to return to NYU from the Sloan Kettering Institute to which I had gone to join David Pressman on a study of antigens in normal and tumor cells. As I recall the circumstances, the invitation grew out of a grant from the Standard Oil Co. of New Jersey to the Institute for research in immunology involving skin. The grant provided \$10,000 per year and it was intended to support the research and to provide a stipend for a young faculty person, with the understanding that the stipend could be augmented because of its limited nature. When offered the grant, I accepted without hesitation. The amount it provided seemed a princely sum at the time; and the need I faced to support a growing family seemed also to be provided for by the opportunity to practice medicine part-time in mid-town Manhattan, in an office that was a 10-min walk from the laboratory. The laboratory was then located in handsome quarters in the then new medical sciences building on 1st Avenue.

I have never for a moment regretted accepting that offer. As an assistant professor, I was completely independent to develop a research program in immunology; and the stipulation that the skin be involved was no restriction at all, for it fit very well with my intention to depend upon allergic skin reactions to simple chemicals as an assay system for examining the structure-function relationships in immune responses to haptens. My move to the Institute proved all the more congenial where I was joined by a group of effective enthusiastic colleagues. Sid Belman was nominally a technician, but quickly became a research colleague, then a graduate student, and later (after I had gone to St. Louis) a member of the faculty. Leo Orris, and then Mary Carsten and Milton Tabachnick as

postdoctoral research fellows, and Bernie Levine, who was then a medical student, completed our small but congenial and effective group.

The focus of the work was the hypothesis, advanced in about 1910, that simple chemicals induced immune responses by combining *in vivo* with tissue proteins to form complete antigens. Extensive studies by Landsteiner and his colleagues supported this view by showing consistent correlations between a simple chemical's activity as an immunogen and its reactivity with aniline, taken to represent tissue proteins. Our point of departure was chromatography—then a novel technique. With the aid of this technique we were able to bring further evidence in support of the hypothesis. In addition, we could identify the particular amino acids of skin protein that reacted with single sensitizers. Some unexpected consequences of these studies led to the development of simple and rapid ways to prepare soluble 2,4-dinitrophenyl (DNP)-proteins, to purify anti-DNP antibodies, to measure affinities of these antibodies for ligands, and to help establish the DNP hapten (and latter 2,4,6-trinitrophenyl [TNP]) as useful antigenic systems for analyzing many problems in cellular and molecular immunology.

These developments were rewarding and would have been totally satisfying, except that the growing attractiveness and demands of the work made it increasingly difficult to meet the responsibilities and obligations of even a small office practice, limited at first to late hours in the afternoons and then to late evenings.

The resulting conflict was resolved when W. Barry Wood, then head of the Department of Medicine at Washington University School of Medicine, St. Louis, appeared in the Institute one day to ask whether I would be interested in a full-time position as professor of medicine at that institution and head of dermatology at Barnes, its teaching hospital. Dr. Wood, a trustee of the Rockefeller Foundation, had persuaded the Foundation to endow a chair for dermatology at Washington University. Though I had no training at all in skin diseases, I evidently was offered the chair because immune skin reactions figured so prominently in our work. The offer was especially attractive, and was finally accepted, because it meant an end to the irreconcilable demands between a growing research program and the insistent obligations of a medical practice which, though small, could not be ignored.

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I mention so many personal details to make it clear that my life in laboratory investigation really began at the Institute, and its origins there I ascribe to Norton Nelson's influence. Whatever his official title may have been at the time, he was a *de facto* director of research. And he was, and is, a model of what a research director should be. With wide-ranging interests and curiosity, and a critical intelligence, he was quick to appreciate the significance of research in areas as arcane as immunology was at the time, and as remote from his background in classical biochemistry. His cheerfulness and optimism were supportive when the work was going well and comforting when it did not.

Ed Palmes and Bernie Altschuler shared these qualities and reinforced them, and together with Norton helped create a laboratory climate that I remember with pleasure and gratitude. It was stimulating, yet critical, and unfailingly balanced by a level of civility and congeniality that I have not seen exceeded anywhere.

In retrospect, one of the remarkable features of the Institute was its self-generation. Having at the time no official teaching obligations and no research tradition to fall back on, the Institute was established—it seemed to me—because Anthony Lanza and Norton Nelson had the foresight to recognize the need for scientific study of industrial and environmental biomedical problems. The origins and subsequent maturation of the Institute represent an entrepreneurial spirit that, while expected and honored in industry, is remarkably rare in academic institutions.

No wonder that I have always looked back to the time I was privileged to spend at the Institute with pride and pleasure. To be able to contribute to the ceremonies that commemorate 40 years of its growth and development, and to Norton Nelson's central role in it, provides me with the opportunity to acknowledge a sense of gratitude that I have long felt, but not previously had the opportunity to express.